OBSERVATIONS ON SILPHINÆ WITH A NOTE ON INTRASPECIFIC VARIATIONS AND THEIR DESIGNATION

By Melville H. Hatch

The following observations on Silphinæ are supplemental to two previous papers on the same group.[1] I am indebted to Mr. Roy D. Shenefelt for permission to study the collection of Washington State College.

Apteroloma tenuicorne LeC.—While I have taken this species from as low an elevation as 1900 feet at Cle Elum, Washington, and Mr. M. C. Lane has taken it at about 1800 feet at Ritzville, Washington, it appears to be especially prevalent at somewhat greater elevations in extreme eastern Washington and northern Idaho. Thus I have a series of thirteen specimens taken by Mr. Lane at an elevation of about 4000 feet on Cedar Mt. near Moscow, Idaho, on May 25, 1935; and Mr. Clifford J. Burner and I secured fifty or more specimens on May 30, 1937, at an elevation of from five thousand to fifty-two hundred feet on Mt. Spokane, northeast of Spokane, Washington. In this latter situation the beetles occurred on the damp or wet ground under the edges of the melting snow fields.

Silpha (*Thanatophilus*) *trituberculata* Kby.—A second Washington specimen was taken by Mr. Joseph Bruzas near the Dry Falls of the Grand Coulee, May 7, 1938, and given to me.

Silpha (*Thanatophilus*) *lapponica* Hbst.—There are, in the Washington State College collection, two series of this species taken in Chicago by A. L. Melander and in "N. Ill." by F. M. Webster. This extends the range of this species in this direction and makes its absence from Indiana and southwestern Michigan more noteworthy than ever.

Silpha (*Blitophaga*) *opaca* Linn. and *bituberosa* LeC.—Essig[2]

[1] Hatch, Melville H. Studies on the Silphinæ. Jour. N. Y. Ent. Soc., XXXV, 1927, pp. 331–371.

Hatch, Melville H., and William Rueter, Jr. Coleoptera of Washington: Silphidæ. Univ. Wash. Publ. Biol. I, 1934, pp. 147–162.

[2] Ins. w. N. Amer., 1926, pp. 381–383.

lists the first of these species from Alaska to California and the second from Washington. I have no knowledge of either occurring in Washington.

Nicrophorus (*Necropter*) *vespilloides* Hbst. (*defodiens* Mann. of Portevin and Hatch).—I follow Leech[3] in his inability to distinguish specifically the Nearctic and Palæarctic forms of this species and in his use of *defodiens* Mann., as a Pacific Coast subspecies of the same. Leech's notes are incorporated in the following revised tabulation of the Nearctic forms of this species, which should be used in connection with my original table:[4]

vespilloides Hbst. (*defodiens* Mann. of Portevin and Hatch)
- typical form (*hebes* Kby., *pygmæus* Kby., *humeralis* Hatch)
- ab. *ruber* Hatch
- ab. *nearcticus* nov. (*defodiens* typical form Port. and Hatch, nec. Mann.)
- ab. *nicolayi* Hatch
- ab. *oregonensis* Hatch
- subsp. *defodiens* Mann.
- typical form (*nunemacheri* Hatch, *nunenmacheri* Leech)
- ab. *binotoides* nov. (*binotatus* Hatch nec. Port.)
- ab. *conversator* Walk. (*defodiens* var. b of Mann., *lateralis* Port.)
- ab. *pacificæ* Hatch
- ab. *gaigei* Hatch
- ab. *walkeri* nov. (*conversator* Port. and Hatch, nec. Walk.)
- ab. *kadjakensis* Port.
- ab. *mannerheimi* Port.[5]
- ab. *binotatus* Port.[6]

It should be noted that, with the exception of the typical form, none of the Nearctic aberrations of *vespilloides* occur in the Palæarctic Region and that, moreover, the types of variation in the two regions are very different. None of the Palæarctic aberrations, for instance, appear to lack an orange spot from the base

[3] Bull. Brook. Ent. Soc., XXXI, 1936, p. 156.

[4] Hatch, JOUR. N. Y. ENT. SOC., XXXV, 1927, p. 356.

[5] The anterior elytral fascia in this aberration is divided into two. I was in error in describing it as constricted.

[6] The posterior elytral spot is absent, the anterior fascia reduced to a single spot in this aberration.

of the epipleuron,[7] which is lacking in all the American aberrations except the typical form and the ab. *ruber*. Leech[8] suggests that the subsp. *defodiens* extends to Japan, and the ab. *sylvivagus* Reitter from that locality may be referable to that subspecies, apparently resembling the ab. *lateralis* Port. On the other hand, *sylvivagus* is, according to Portevin,[9] related to the eastern Asiatic varieties, *borealis* Port. and *sylvaticus* Reitt., in which the abdominal pubescence is yellow rather than black.

Leech's observations[10] on the habits of *defodiens* are of great interest. They confirm to a suprising degree those previously published by Pukowski[11] for a series of European species, including *vespilloides*. These observations of Pukowski's are abstracted at length in English by Balduf in the *Bionomics of Entomophagous Coleoptera* (John S. Swift and Co.), 1935, pp. 69–75.

Nicrophorus (*Necropter*) *investigator* Zett. subsp. *investigator* Zett. and *nigritus* Mann.—I have now seen examples of both these forms from Pullman and the latter in addition from Spokane and Wawawai, all in eastern Washington.

Nicrophorus (*Necropter*) *guttulus* Mots.—Of the subsp. *guttulus,* I have a single example of the ab. *vandykei* Angell from Pullman and ab. *woodgatei* from Port Townsend, both in Washington.

Intraspecific variations and their designation[12]

Intraspecific variation should be studied for the following reasons:

(1) The fundamental obligation of taxonomy is to describe accurately and precisely the variation presented.

[7] This term is, according to Torre Bueno's *Glossary of Entomology*, 1937, pp. 92, 132, definitely superior to "*hypomeron*," which I used formerly.

[8] *L.c.*, p. 156.

[9] Bull. Mus. Paris, XXX, 1924, p. 375; Encycl. Ent., VI, 1926, p. 235, 259.

[10] Proc. Ent. Soc. B .C., XXXI, 1935, pp. 36–40.

[11] Zeit. Morp. ökol. Tiere, XXVII, 1933, pp. 518–586.

[12] These remarks are supplemental to my original statement in Jour. N. Y. Ent. Soc., XXXV, 1927, p. 341, and are largely prompted by Leech's comments on my classification of the aberrations of *Nicrophorus vespilloides* subsp. *defodiens* Mann. in Proc. Ent. Soc. B. C., XXXI, 1935, pp. 36–40, and Bull. Brook. Ent. Soc., XXXII, 1937, pp. 156–159.

(2) What appear at first to be intraspecific variations are frequently the elements out of which subspecies and species are later recognized. The indication of such forms by one author, though he holds them to be entirely intraspecific in nature, may enable a subsequent author to reach conclusions of a very different sort.

(3) Many intraspecific variations are the materials out of which new species evolve in the course of geologic time. Their detection and study is the first step in their consideration as factors in evolution. To argue that this is more suited to genetics than to normal systematic entomology[13] is to adopt a wholly untenable view of the discreteness of taxonomy from other branches of biological knowledge. It is in important measure the taxonomist passing in review large series of different life forms who is in a position to discover this sort of data.

There are several ways in which intraspecific variation may be treated.

(1) It can be entirely ignored! Every working taxonomist can probably bring to mind descriptions in highly variable groups which mention only the supposed specific characters with never a word as to the variation.

(2) Then there is the traditional method, which is a prevalent procedure in America, of devoting a special paragraph to the intraspecific variation. This method is applicable to strictly continuous variation, where it can be supplemented by curves of variation and other graphs. It is also useful where the variation is vague or extremely complicated. When applied to more or less discontinuous variation, it shows that the study of the variants is in an early pre-Linnæan stage, since subsequent references to the variations mentioned must repeat the entire definition of the variety.

(3) The early coleopterists lettered intraspecific variations. Thus Illiger, in his *Verzeichniss der Käfer Preussens* (1798), and Schönherr, in his *Synonymia Insectorum* (1806–1817), and LeConte as late as 1880 gave Greek letters to their varieties, "alpha" being reserved for the typical form. The current pro-

[13] Leech, *l.c.*, p. 39.

cedure would undoubtedly be to substitute Latin for Greek letters, and I have employed letters myself on several occasions.[14]

(4) The transition from letters to names was gradual. One author would describe a species which another author would regard as a variety. The extra name would be retained as a varietal name and the tendency would be to invent new names for other varieties of similar rank.

For about half a century continental coleopterists have employed the term *aberratio* (English, *aberration*) to apply for the most part to color varieties. Most of their valid species were described, so they turned to the problem of intraspecific variation. Aberrations were used by Ganglbauer in *Die Käfer von Mitteleuropa* in 1892 and are to be found employed in practically every continental work on coleopterology that has appeared since 1900, but only during the past decade has this usage made much headway in English speaking countries. Leech[15] finds the term so unusual that he puts it in quotation marks and there is an unfortunate tendency[16] in some quarters to substitute the English for the Latin meaning of the word and thus limit it to freakish, monstrous specimens. In reality the word is to be derived from the figurative use of the verb *aberro,* "to wander, deviate, depart from."

There has been a general tendency to regard all intraspecific names as being nomenclatorially on a par with specific and subspecific names,[17] and this is, perhaps, one of the chief sources of the prejudice against them. Many systematists have disliked seeing names based on trivial or supposedly trivial features on a par with names based on supposedly fundamental characters. There is, accordingly, some cogency for:

(5) The procedure suggested in the *Entomological Code* of Banks and Caudell (1912, p. 9, sec. 37) and that of the British National Committee on Entomological Nomenclature[18] releasing

[14] Tech. Publ. N. Y. St. Coll. For., 17, 1924, p. 307; Univ. Wash. Publ. Biol., I, 1932, p. 100.

[15] Bull. Brook. Ent. Soc., XXXII, 1937, p. 158.

[16] Gunder, Ent. News, XXVIII, 1927, p. 265; Carter, Ann. Mag. Nat. Hist., 104, 1934, p. 552.

[17] This is my own preference.

aberrational names from the application of priority. The British code would release them entirely, the Banks and Caudell code would require priority to operate within the limits of the single species.

(6) The logical consequence of releasing aberrational names from the operation of priority are the somewhat similar proposals of Blaisdell[19] and Heikertinger[20] who suggest the use of descriptive or semi-descriptive words in place of aberrational names, these words being used over and over again in different species even of the same genus as often as required, being purely descriptive, and absolute synonyms of ordinary specific and varietal names entirely free from the operation of priority or other nomenclatorial rules. Blaisdell called the category to which he applied these names a "forma," employed words like *typica, glabra, interstitialis, punctata, annectans, catalinæ, communis, emarginata, borealis,* etc., and accompanied them with descriptions. He used them in connection with his monograph of the Eleodini referred to above, and where it has become subsequently desirable to elevate some of them to the rank of variety or species, he considers[21] the names so used to date from the time of their elevation rather than from that of their original proposal.

Heikertinger calls his terms "Kennworten," "recognition words," and has attempted to use words of self-evident meaning, not requiring attendant descriptions. He works out this suggestion in the Halticinæ section of Winkler's *Catalogus Coleopterorum regionis palæarcticæ* (1930, pp. 1317–1352), using terms like *viridicollis, ruficollis, vitipennis, maculipennis, nigrosuturata, latilimbata,* etc. Heikertinger's system would appear

[18] Proc. Ent. Soc. London, III (1), 1928, p. 11R.

[19] U. S. Nat. Mus. Bull., 63, 1909, pp. v–vi.

[20] Kol. Rund, XV, 1930, pp. 213–230. In this connection it is interesting to note the suggestion of Croneis in Science (LXXXIX, 1939, pp. 314–315) of a series of categories paralleling the Linnæan ones for use in classifying fossil remains whose true biological affinities are not ascertained. This is significant as an insistence that taxonomy not neglect its basic function of describing, classifying, and designating for the important but sometimes impossible task of interpreting. Hubbs' arguments (Science, LXXI, 1930, pp. 317–319) in favor of a uninomial as opposed to a binomial system of species designation is another suggestion looking in the same direction.

[21] Pan-Pac. Ent., II, 1925, p. 77.

to be applicable only to relatively simple types of variation rather than to those involving complicated color pattern changes.

(7) For complicated color pattern variations formulæ can be used. The spots or other elements of the pattern are numbered or lettered, various combinations of these symbols indicating the different forms. This system was used by Johnson in Coccinellidæ[22] and I have myself employed it on several occasions.[23]

(8) The ultimate stage in the nomenclature of intraspecific variation is attained when it can be defined in terms of the constituent genes or other hereditary or environmental factors involved. This is possible at present for only a very few species, like some of the *Drosophila* and a few of the Chrysomelidæ and Coccinellidæ.

The principal requirement of a nomenclature for intraspecific variations is that it be clear cut, unequivocal, and of such a nature as to make it possible for subsequent authors to refer to the forms described precisely without having to repeat the entire description in pre-Linnæan fashion. Any one of the last six methods cited is available with the second one in reserve for vague or imperfectly understood variation. Whether one uses numbers, letters, formulæ, or names with or without priority, or all in combination, is of secondary importance so long as is met the initial requirement of precision.

Objections to studies of intraspecific variation are voiced on various grounds. It is said that many variation studies, especially those involving scarcely more than the pointing out of the existence of the several variations, are of little value because of their superficial preliminary nature, because they are confined to a single prominent variable character, or because no attempt is made to correlate them with the environment or heredity of the form under consideration. One might equally criticize the describer of a new species because he fails to work out its life history or genetics before going on to the next species. These problems are important and some day will be studied by somebody. But the task, or one of the tasks, of taxonomy is a descrip-

[22] Johnson, Roswell H. Determinate evolution in the color-pattern of the lady-beetle, Carnegie Inst. of Wash. Publ. No. 122, 1910, 104 pp.

[23] JOUR. N. Y. ENT. SOC., XXXV, 1937, pp. 347–348; Univ. Wash. Publ. Biol., I, 1932, pp. 98–99.

tive survey of the animal kingdom—a survey that would be indefinitely delayed if each investigator attempted to see each of his problems through to an ultimate ecological or genetical conclusion. It is stated, again, that such studies are "genetics" rather than "normal systematic entomology"! They are criticized because they are of no use to the economic entomologist.[24] Apparently "normal systematic entomology" may "make up" to the economic entomologist, but must "shy off" from the geneticist!

One of the main results derived from designating a form an aberration or color variety or forma is that it is thereby almost certainly removed from the attention of the economic entomologist and the general ecologist,[25] except as they find it convenient not to be led astray by extreme phases that the species may assume, melanistic, immaculate, or depauperized forms that might at first be mistaken for distinct species. The classification of aberrations is usually of no more significance for such persons than, for instance, the designation of sex; in fact, it may be of far less importance, since the recognition of the sexes is of practical concern in many experimental procedures. Thus, in citing an insect like the asparagus beetle, *Crioceris asparagi* L., for the *Insect Pest Survey Bulletin,* it would be absurd to give more than the species. The numerous aberrations of the asparagus beetle[26] and other species are available for those who need or are interested in such matters; they may be overlooked by others.

A more cogent objection to the validity of studies in intraspecific variation is that such variations may intergrade; they may intergrade in series, the right and left side of the body may exhibit different variations, different variations may occur in the progeny of the same pair of parents.[27] A good deal of the force of this objection is due to a misapprehension. The principal difference between species and intraspecific forms of all sorts is that the

[24] Hopping, Proc. Ent. Soc. B. C., XXXI, 1935, p. 34.

[25] The person making a detailed ecological study of a single species may be very much interested in aberrations, especially if any of them prove to have an origin that is immediately environmental.

[26] See Hatch, Bull. Brook. Ent. Soc., XXII, 1927, p. 211; Univ. Wash. Publ. Biol., I, 1932, pp. 72–74, for some account of these aberrations and their literature.

[27] Leech, *l.c.*, p. 158.

former are characterized by an absence of intermediates, at least in theory. If the intraspecific variations were marked off as distinctly as it is intimated they should be, they would not be intraspecific variations but species.

The matter, then, comes to this: Is it worth while to attempt to distinguish characteristic phases of a more or less continuously varying series? And, if we do, what is to prevent us from running off into non-significant "unnamables" and individual variants? The question of "unnamables" in Lepidoptera has been discussed by Gunder[28] and Forbes[29] and should offer little difficulty in the long run. Unless the causes of the variation have acted acted from within the living organism,[30] the forms produced are clearly of little significance[31] for the student of intraspecific variation. Moreover the variation must be of such a type that it occurs or probably occurs in series. Otherwise it is "individual" or "unnamable." Even as regards size, it is sometimes convenient to designate minor or depauperized specimens, especially when this is accompanied by the specific characters in an enfeebled form.

Leech (*l.c.*) specifically questions the utility of distinguishing four stages in the disintegration of the anterior elytral fascia in one of the Pacific Coast forms of Nicrophorus. In 1927 I described these and other aberrations of this species on the basis of series too small to show their nature. In 1934,[32] in a series of 178 specimens from western Washington, I showed that these four stages form an approximately normal curve of variation with the apex of the curve close to one of the intermediate types. Leech could have continued the analysis in other portions of the range. He preferred, however, simply to express the view that such a study, based on color pattern variation alone, was without signifi-

[28] Ent. News, XXXIX, 1928, pp. 201–204, pl. VII–X.

[29] Bull. Brook. Ent. Soc., XXIX, 1934, pp. 65–67.

[30] By which I do not mean to limit the causes to hereditary ones. Environment may, for instance, produce melanism, which I would tend to regard as "significant."

[31] I prefer "significance" to "nameability" because, throughout the present discussion, I maintain that whether a variant is "named" or merely designated by a number, letter, or formula is of no importance.

[32] Univ. Wash. Publ. Biol., I, 1934, p. 158.

cance, and a large number, perhaps the majority, of American coleopterists would agree with him.

The principal objection to studies of intraspecific variation is not directed against the studies as such but against the *naming* of the variations. As has been noted, the author's contention is that whether or not the variants are named is a matter of secondary importance, so long as they are precisely designated. This, however, is not the view of opponents of names of this class. They consider the so-called "cluttering up" of the literature with such names an offense than which scarcely any other is greater! as though *naming* a variant could give it any importance and dignity it did not have before! Names are merely combinations of letters—symbols for designating biological concepts. They are symbols, however, that, because of their great associational powers, are so much more convenient than letters or numbers or formulæ that the tendency to use them for this class of variation is very great. They, varieties, color varieties, aberrations, forms —the precise term applied is of little moment—named or merely designated—should be taken at their face value for exactly what they are—categories intermediate between the subspecies and the individual.

Where then are we going to stop? Lacking data derived from genetical experimentation, we must depend on our judgment. The benefit of the criterion of the presence or absence of intermediate specimens is denied to us, since all intraspecific variations are subject to such a connection. Forbes (*l.c.*) suggests that if we can recognize 90 per cent of the specimens from a given area as belonging to a given geographical race the race is valid. Perhaps that or some other percentage will serve for the recognition of aberrations. The matter is really akin to that of genera and subgenera. As many are recognized as are convenient. And the number, as with the higher taxonomic categories, is almost certain to increase as more and more extensive series of specimens are passed in review and more and more precise methods of study are employed.

There seems to be concern in some quarters that a continued accumulation especially of named varieties, aberrations, etc., will involve the literature in hopeless confusion. Such a fear is

groundless. The matter is self-correcting, just as is the over multiplication of categories at any of the other systematic levels. Only such designations persist as subsequent authors find recognizable and useful. Authors who do not care to pay attention to non-geographical intraspecific variations ignore them, treating such names as may have been proposed as *absolute synonyms,* which, indeed, they truly are, *from their point of view.* Other authors, who are interested in such categories, recognize as many of their predecessor's "designants" as the nature of the characters cited in the original descriptions or inherent in the types, where types are available, and the nature of the organisms themselves permit—exactly as with categories of other types. It is probably too much to expect that there will be any cessation of vituperativeness between persons holding different views!

The whole matter may come down to personal preference. But the growing continental usage would seem to indicate that, as the beetle fauna becomes better and better known, more attention is directed to intraspecific variation.

Studies of these intraspecific categories may be largely overlooked by the economic entomologist and the ecologist, and must be overlooked by the naturalist whose aim is simply to get into his cabinet a "set" of two, six, or any other fixed number of specimens of each species. They are, however, almost inevitable for the student who desires to study his material with some regard for its complexity—an inadequate forecast of the time when taxonomists will analyze their species genetically.

SUMMARY

It is held that intraspecific variants, such as varieties, color varieties, aberrations, forms, etc., should be as precisely described and designated as the material will permit. Whether this designation is accomplished by means of numbers, letters, formulæ, or names with or without priority is secondary. The author prefers to allow the principal of priority to operate as generally as is conveniently possible, but this likewise is a secondary consideration.

I must not close this discussion without acknowledging the invaluable critical assistance I have received in its preparation

from two of my former students, Dr. Donald L. Frizzell and Dr. Harriet Exline Frizzell, especially the former, neither of whom, however, are responsible for any of the opinions expressed herein.

A NEW RECORD FOR CONNECTICUT

Along a wooded road near Sharon, Conn., on June 16, 1940, a small skipper, *Carterocephalus palæmon,* race *mesapano* Scudder was discovered. This is a rather unusual record for this butterfly which hardly ever ocurs south of the Adirondacks or the White Mountains. So far as is known, this is the one and only instance that it has ever been found in Connecticut.—LEONARD J. SANFORD.